

SCIENCE.

FRIDAY, SEPTEMBER 25, 1885.

COMMENT AND CRITICISM.

THE NEWS-REPORTERS of the daily press are trying to enliven the dull season by the invention of stories of fraud and irregularities in the various scientific bureaus of the government. Now it is a new find in the accounts of the coast-survey, and now it is a startling report upon the proceedings of the fish-commission, or the work of the national museum. These stories have the appearance of being earnest endeavors to guess out things which, from the point of view of the news-reporter, might possibly be true, made by men who do not know enough about the case to make their guesses even plausible. Last week the *Boston Advertiser* published a despatch from Washington, purporting to give an abstract of a report on the operations of the geological survey. The despatch might have passed muster but for an unfortunate endeavor of the reporter to strike beyond his reach, by saying that Professors Cope and Marsh had been allowed to appropriate collections of the value of \$150,000, belonging to the survey. Had he known that Cope never had any connection with the survey, and so had never had an opportunity to possess himself of its specimens, the inventor would, no doubt, have substituted some other name for his. That a board of experts would accuse the director of the Peabody museum of Yale college of unlawfully possessing himself of specimens handed to him for examination and report, is a statement fitted only to provoke a smile. The fact that the director of the survey had just left for a month's tour of inspection in the far west, rendered almost unnecessary the denial of the auditor that such a report existed.

When the history of the irregularities in the coast-survey is calmly reviewed, it will, no

doubt, be found that the interests of the government have suffered less than one would suppose from the flaming head-lines which introduced the treasury report on the subject. If, as is said to be the case, the work unlawfully paid for was all done outside of office hours, and in addition to the regular duties, the case may admit of some palliation from a moral aspect, even though we deplore such a departure from the spirit of the wise and necessary law that salaried government employees shall not receive extra pay for real or supposed extra work. The country will not see any great wrong in loaning antiquated transit instruments to institutions of learning, when they no longer serve the purposes of the survey. The disposition to show a newly discovered evil in the strongest light, and to omit palliating circumstances, is common to all men, committees of investigation included. Let us, then, reserve our judgment until we have heard and weighed the other side.

THE PUBLICATION of his third report on the insects of Illinois furnishes the state entomologist, Dr. S. A. Forbes, an opportunity to prepare an index to the first twelve reports, which is done in excellent manner, and to call attention to the fact, that, in volume, the fourteen reports of the entomologists of Illinois, amounting in all to 2,358 pages, exceed the literature of economic entomology of any other state. Commenced by Walsh, who died soon after his appointment, and continued by Le-Baron and Thomas, the office has now fallen into better hands than at any time since its foundation, the reports of Mr. Forbes having already become a standard by reason of the independent and original methods with which he is pursuing the study of economic entomology. The three reports which he has now published, equal in value the larger bulk of their predecessors.

LETTERS TO THE EDITOR.

Recent volcanic activity in the United States: eruptions of Mount Baker.

In a late communication to *Science* from Capt. Dutton of the U. S. geological survey, he mentions the date 1850 as that of the latest volcanic eruption within the limits of the United States. This must necessarily exclude Alaska. He also refers to vague reports of volcanic activity at later dates.

In adding a mite to the subject of his paper, I would ask your permission to recall three instances of volcanic activity on this coast, leaving open the question, What is the exact line between the smoldering of the 'living' volcanoes, and those which are 'active,' or in 'eruption' ? etc.

1°. In 1854 I was one day observing at the trigonometrical station on Obstruction Island in the Rosario Strait, Washington Sound: I had finished the measures for horizontal direction of the summit of Mount Baker, and was commencing a series of measures of the vertical angles for elevation, when I found the whole summit of the mountain suddenly obscured by vast rolling masses of dense smoke, which in a few minutes reached an estimated height of two thousand feet above the mountain, and soon enveloped the higher parts. Baker was distant thirty-nine geographical miles from my station, and bore about north seventy-five degrees east, true. The weather was fine, and we hoped to see a brilliant display at night; but unfortunately the sky clouded, and we could not see the light at night, nor the mountain next day: when the weather cleared, the eruption had ceased; and, instead of the white mountain mass, we discovered that the snow covering it was apparently melted away for two or three thousand feet below the two heads. Of course the snow may not have been melted, but only covered with ashes and scoriae; and we had not the means of deciding the question at that distance.

We had been in those waters during parts of 1852 and 1853, and then the snow-clad mountain was quiet.

We discovered that the crater was not on the summit, or on the secondary peak to the south-eastward, but on the flank of the higher peak, and opening towards the south or south-west. In subsequent years we occasionally saw small volumes of smoke issuing from this crater. The facts of this eruption were reported by me at the time.

2°. In 1858 Mr. John S. Hittell of San Francisco was in Victoria, and he informs me that the night clouds over Mount Baker were brilliantly illuminated by the light from an eruption of Mount Baker. Upon his making inquiries among the citizens of Victoria, they expressed themselves as being well aware of the eruption then going on.

3°. I left these waters at the close of the season of 1857; but my colleague, Capt. James S. Lawson, who succeeded me in that section, says there was no eruption for the years he observed in the Gulf of Georgia. In 1867 and in 1869 I passed through the waters of Washington Sound and the Gulf of Georgia, and returned, all in sight of Mount Baker; but there was nothing unusual in the appearance of the mountain. In 1870, when I was passing through Admiralty Inlet and the Strait of Fuca, towards Victoria, Mount Baker was very clearly in sight at about sixty miles distance, when I beheld great volumes of smoke projected from the crater to an estimated height of eight hundred feet above the higher peak. During my stay at Victoria in September, with Mount Baker distant seventy-three and three-quarter geographical miles, and bearing north seventy degrees east, true, from Rocky Hill, I made observations for the height of the two

peaks, the position and size of the crater, and the height of the snow-line. I made also an accurate drawing of the outline of the mountain and its surroundings, the more particularly because rumors had found their way into the newspapers, asserting that the summit of Mount Baker had fallen in. On the contrary, I was perfectly satisfied, from my years of familiarity with its features, that no such catastrophe had taken place between 1852 and 1870; nor was I able to detect any changes in 1877, when I was daily in sight of Mount Baker for some time.

I should call attention to the fact, that, a good many years since, Mr. E. T. Coleman of Victoria (if I remember his name properly), after two unsuccessful attempts in two different seasons, was the first alpine climber who made the ascent of Mount Baker. He published his account thereof, with illustrations of the glaciers, névé, etc., in *Harper's magazine*; but I cannot recall the date. I add the following data, which were incidentally obtained in different years by Capt. Lawson and myself when engaged in regular coast-survey duty:—

The geographical position of the higher and main peak of Mount Baker is, latitude, $48^{\circ} 46' 34''$ north; longitude, $121^{\circ} 50' 4''$ west. The height is 10,755 feet above the sea level; the height of the second peak, which lies probably two miles towards the south-east, is 10,163 feet; the upper part of the crater is 596 feet below the summit of the mountain; and the length of the crater about 400 feet. The crater appears to be four times as long as it is broad; the narrowest part is the upper limit; and it lies on the southern slope of the main peak, and parallel with the slope, which I judged to be thirty degrees with the horizon. The lowest limit of the snow-line in September, 1870, at the close of the summer dry season, was 5,301 feet above the sea.

These observations were made with a small instrument at a long distance; but my height of the principal peak differed only seventy-two feet from the mean of previous observations, and I believe they are trustworthy.

GEORGE DAVIDSON.

Davidson observatory, San Francisco, Cal., Sept. 1.

Linguistic studies at the Siletz agency.

In the abstract of my first paper read in Section H., A.A.A.S., as published on p. 230 of *Science* for Sept. 11, a slight mistake was made. For 'three sets of cardinal numbers, human, inhuman, and inanimate,' read 'two sets of cardinal numbers, the human, and the non-human series.'

J. OWEN DORSEY.

Washington, D.C., Sept. 19.

Spectrum of the great nebula in Andromeda.

By employing the eye fresh from some hours' sleep, by looking for some time in the spectroscope, and by

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giving the spectrum a slight apparent to-and-fro motion, I have been enabled, independently, upon Sept. 5, 7, 9, 12, 21, to discern three bright lines superposed upon the continuous spectrum of the nebula in An-

dromeda. The relative positions are shown in the adjoining cut.

The 'lines' at 18.6 and 19 appear, using the broad slit, as bright knots. That at 17.5 as a long line. The arrangement of the lines suggests certain bright lines in the spectrum of γ Casseopeiae and β Lyrae, and the settings agree with those made upon the spectrum of the said stars.

O. T. S.

Yale college observatory,
Sept 14.

The Mexican axolotl and its susceptibility to transformations.

Marie von Chauvin's experiments with the axolotl, as recounted in *Science* No. 130, under the above title, interested me very much indeed, inasmuch as they came upon me at a time when I was experimenting with upwards of two hundred of these animals by very similar methods.

My present field of research is in north-western New Mexico, and several forms of axolotls are to be found in the region. Last June (1885) I visited, near my present residence, on more occasions than one, a small pond that contained large numbers of them. This pond is nearly square, and its sides something over a hundred feet in length. It is divided in two nearly equal parts by an east and west embankment. This embankment has a narrow trench cut through it, so that when the rain fills the ponds they communicate with each other; but this is not the case when the water is low.

By the 1st of September each year these ponds are usually dried up; while during the spring and summer months, the south one has a mid-depth ranging between three and six feet, and the north one being considerably shallower. These depths vary with the amount of rainfall, and other meteorological conditions.

As I say, there were great numbers of axolotls in these ponds; and as far as I could see, and by the very kind assistance of Professor Cope, these were of two kinds: one very large one (20 cms \pm) seemed to be the larval form of *Amblystoma mavortium*; another much smaller one (9 cms \pm) proves to be *A. tigrinum*. In addition to these, there are some medium-sized ones that are very puzzling, and not yet satisfactorily diagnosed. With but few exceptions, the north division of the pond contained the small ones; while in the other side lived all the large ones, together with the great majority of the light-colored and undetermined forms.

The limits of this paper will not permit me to present all the conditions of environment under which these axolotls live, much less an account of the many observations I made upon their habits as they are to be seen in a state of nature. At different times I captured as many of these creatures as I desired, to carry on my experiments at home, the results of which I had the unusual opportunity of comparing with those changes undergone by these reptiles while existing in their natural element.

It is my sole aim in this paper to briefly present the results of some of these experiments, so far as they have gone, and compare them with those arrived at by Miss von Chauvin, as set forth in *Science*.

My observations confirm those of this talented authoress, in that, —

1. Axolotls are more readily converted into *Amblystoma* if kept in water containing but little air, and *vice versa*.

2. If transformation is forced up to a certain point

in development, the reptile arrives at the higher form without any further interference.

3. Axolotls live in the water with apparent comfort a considerable and varying length of time after their gills have been absorbed.

4. After the metamorphosis is completed, their power to return to the water again to live, seems to depend upon the moult, and whether they have lived in moist or dry places since the metamorphosis.

5. By varying the conditions under which these animals live, we can at our pleasure retard or accelerate their development to the higher stages.

6. Young axolotls are more easily transformed than the older specimens, but this rule also depends largely upon the conditions under which these animals live.

There is another very important factor that enters into this metamorphosis, that, so far as the account in *Science* goes, is not touched upon; and that is, the question of their diet during the experiments. Axolotls are very voracious creatures, and eminently carnivorous. They are very fond of raw meat; and, upon the slightest provocation, they will feed upon each other. So I have found, during the course of my experiments, that, —

7. The metamorphosis is hastened by regularly supplying the animals with plenty of proper food. And what is still more interesting, when they are thus treated, it markedly affects the appearance of the transformed *Amblystoma*.

8. If, during the process of forcing the transformation of axolotls, the animals are regularly supplied with the requisite amount of fresh meat, the transformed *Amblystomas* are very much larger and stronger than those which are transformed without having received any food. In the case of *A. tigrinum*, those that received food, the transformed animal would hardly have been recognized as the same species: they were not only larger, but of a very deep, muddy, black color, without spots; while the others were mottled with bright yellow, and a pale brown.

9. The depth of the water has a wonderful influence upon the metamorphosis; and the fact is well known, that, the deeper the water in which the axolotls live, the slower their transformation.

Temperature is another important factor in the change, and its moderate increase seems to hasten the transformation.

Now, the most interesting part of all is to watch the operation of these laws, that I have given, in nature, and the manner in which the metamorphosis of axolotls is there effected.

It would, indeed, be hard to find anywhere a more perfect and beautiful example illustrating the extremely sensitive balance that may exist between the surrounding conditions on the one hand, and their effect upon an animal organism on the other. This year, for instance, the very pond that I have alluded to above, gradually dried up; the north half of it entirely. This took a number of weeks; but during that time all the modifications of which the metamorphoses of axolotls are subject to, or capable of, were, so far as their necessity goes, most lucidly demonstrated. A shallow corner in this pond would, after a torrid day or two, dry up; whereupon all the axolotls that happened to be caught within its limits, would be found, perhaps several hundred of them, under the debris, rapidly assuming the *Amblystoma* form. Numbers of the same generation, however, in the deeper parts, would be unaffected by the change of environment so suddenly precipitated upon their brethren. If the drying-up continued, these trans-

formed animals quit the site, and, during the next few days, could be found under logs, and in other suitable places at some considerable distance from it. On the contrary, should a rain in the mean time fill the pond again, and flood over these shallow parts, the transformations were checked; and those with gills and branchiae in all stages of change, once more took to the water. When huddled together in the shallow places, the large and strong ones devoured the smaller and feebler forms; and the different appearance of the two was very striking upon the most superficial examination.

One day in July the whole north half of this pond suddenly ran dry; and I must confess the sight its bottom presented during the following day was one of the most extraordinary, and at the same time most interesting, that I ever beheld, and, after what has been said, can be better imagined than described. It absolutely swarmed with these creatures, whose organizations were accommodating themselves to the new condition of affairs as rapidly as the laws governing the changes permitted. The study would have furnished food for a small volume.

Axolotls are also affected by the character of the ponds or swamps they live in, the same species showing in different shades in their coloration. Those in shallow ponds with little or no vegetation, and hard clay bottoms, grow to be very light colored, and long retain their larval forms.

No doubt many such ponds as I have described exist all over this south-western country; and a moment's reflection will make it clear to us how the metamorphosis of this creature tends to save thousands of their lives, when the region is visited by a protracted drought, and their places of water resort fail them. The preservation of the form is thereby, to a great extent, protected.

DR. R. W. SHUFELDT.

Fort Wingate, N. Mex.,
Aug. 12.

THE SONG-NOTES OF THE PERIODICAL CICADA.

THERE are few more interesting subjects of study than the notes of insects and the different mechanisms by which they are produced. They interest every observant entomologist; and it is difficult to record them in musical symbols that can be reproduced on musical instruments, some of the more successful and interesting attempts in this direction having been made by Mr. S. H. Scudder. I have studied closely the notes of a number of species, and have published some of the observations.¹

In the notes of the true stridulators more particularly, as the common tree-cricket and katydids, I have been impressed with the variation both in the pitch and in the character of the note, dependent on the age of the specimen, and the condition of the atmosphere, whether as to moisture, density, or temperature. Yet, with similarity in these conditions, the note of the same species will be constant and easily recognizable.

A few remarks upon *Cicada septendecim* will doubtless prove of interest now that the species has been occupying so much attention. I do not find that the notes have been anywhere very carefully described in detail, nor would I pretend to put them to musical scale. Writing seventeen years ago, I described the notes in a general way, as follows:—

“The general noise, on approaching the infested woods, is a compromise between that of a distant threshing-machine and a distant frog-pond. That which they make when disturbed mimics a nest of young snakes or young birds under similar circumstances,—a sort of scream. They can also produce a chirp somewhat like that of a cricket, and a very loud, shrill screech, prolonged for fifteen or twenty seconds, and gradually increasing in force, and then decreasing.”¹

There are three prevalent notes, which, in their blending, go to make the general noise as described above. These are,—

First, That ordinarily known as the *phar-r-r-ao* note. This is the note most often heard during the early maturity of the male, and especially from isolated males or from limited numbers. It is variable in pitch and volume, according to the conditions just mentioned as generally affecting insect melodists. Its duration averages from two to three seconds; and the *ao* termination is a rather mournful lowering of the general pitch, and is also somewhat variable in pitch, distinctness, and duration. In a very clear atmosphere, and at certain distances, an individual note has often recalled that made at a distance by the whistling of a rapid train passing under a short tunnel. But when heard in sufficient proximity, the rolling nature of the note will undoubtedly remind most persons more of the croaking of certain frogs than of any thing else. I have heard it so soft and low, and so void of the *ao* termination, that it was the counterpart of that made by *Oecanthus latipennis* Riley late in autumn, and when shortened from age and debility of the stridulator.

Second, The loudest note, and the one which is undoubtedly most identified with the species in the popular mind, is what may be called the ‘screech.’ This is the note described by Fitch as “represented by the letters *tsh-e-e-E-E-E-E-E-e-ou*, uttered continuously, and prolonged to a quarter or half a minute in length, the middle of the note being deafeningly shrill, loud and piercing to the ear, and its termination gradually lowered till the sound

¹ 3d rep. ins. Mo., 14, 153, 154; 4th do., 139; 6th do., 150–169.

¹ 1st rep. ins. Mo., 24.

expires." Dr. Fitch errs as to the length of its duration; and I have also erred in the same direction — unless, indeed, there is a still greater range than my subsequent observations would indicate.¹ It is more probable, however, that our memories were at fault; for, as I have verified this year, this shrilling ordinarily lasts from two to three seconds, though occasionally longer, and is repeated at intervals of every five seconds. This note is rarely made by solitary males, or when but few are gathered together: but it is the prevailing note in the height of the season, and is made in unison; i.e., the assembled males on a given tree, or within a given grove, are prompted to it simultaneously, so that its intensity becomes almost deafening at times. It is of the same nature as that made by the dog-day Cicada (*Cicada pruinosa* Say), and in its higher and louder soundings is not unlike the shrilling of that species, though by no means so sharp and continuous. It is what in the distance gives the threshing-machine sound, and it has often recalled what I have heard in a saw-mill when a log is being cut crosswise by a circular saw.

Third, There is what may be called the intermittent, chirping sound, which consists of a series of from fifteen to thirty, but usually about twenty-two, sharp notes, sometimes double, lasting in the aggregate about five seconds. This sound is so much like that ordinarily produced by the barn or chimney swallow (*Hirundo erythrogaster*), that a description of the one would answer fairly well for both. It resembles also, though clearer and of higher pitch, the note of *Microcentrum retinerve* Burm., which I have likened to the slow turning of a child's wooden rattle highly pitched. The above notes, so far as I have recognized them, are of higher pitch, but of less volume, in the smaller, *Cassinii*, form.

The other notes — viz., that made when the insect is disturbed, and a not infrequent short cry, that may be likened to that of a chick — are comparatively unimportant: but no one could do justice to the notes of this insect without embracing the three peculiar sounds which I have attempted to describe above, and which are commingled in the woods where the species is at all common; though the undulatory screech is by far the most intense, and most likely to be remembered.

C. V. RILEY.

¹ Since this was written, I have heard, on two occasions, this note prolonged to twenty seconds; but this is quite abnormal, and I have no other evidence than the season (June 20) to prove that it came from *C. septendecim*.

LOST RIVERS.

THE phenomenon of a stream flowing merrily down from a mountain and then disappearing, is, in the west, a very common one. In following down the Rio Grande on an enlarged map, we find many streams entering into it in its upper course. In going down a little farther, reaching the San Luis valley, they are found to suddenly give out on the northern side; and, a few miles farther down, on the southern side also. The principal streams of the valley, the Rio Grande excepted, come in full force down the mountain, flow freely on, and terminate in a marsh, or a small lake, or in the sand. The beds of those which should empty into the Rio Grande are there, but there is no water in them. Similar streams are common over the south-west; and the various streams show all the different stages, from those which really go somewhere all the time, to those which empty into their main stream a part of the time, and those which, alive and full of water above, always fail to reach the stream to which they are headed below.

One time I had the curiosity to examine a stream at the point where it was lost. It was the Rio Hondo, just south of Santa Fé. We had crossed it lower down; and, though the ravine was seventy-five or a hundred feet deep, we had found it perfectly dry. We followed up its south bank for a mile or two until we struck the foot-hills, and there we found it a bright, rippling stream, leaping down from ledge to ledge, very picturesque, with some scattered trees along the banks, and so broad that it was not easy to pass over it, leaping from stone to stone, and remain dry-shod. From here my friend drove back to the crossing, and I walked down to see where and how a stream could lose itself with such a volume of water, and a path well marked out for it. As I followed it down, it ran on merrily in the midst of a little valley not more than six or eight rods wide, along which were pretty meadows alternating with clumps of bushes. It passed through the various incidents of a stream, — here a little fall, there a rapid over thickly set stones, a little farther on a pool. There seemed to be nothing unusual in it, when suddenly I noticed that the little valley widened to double its previous width, the bed became more sandy, and the stream was spread over a greater space. It was evidently going under; and, within twenty rods of where I noticed the first change, the running water had entirely disappeared. The bed of the stream was damp

for twenty rods or so more; then for a considerable distance I could get water by digging a few inches; then that indication failed, and beyond the stream-bed was entirely dry.

Not all such streams terminate thus in the middle of their bed: some terminate in a small shallow lake, some in a marsh; and either lake or marsh is pretty sure to be brackish, due to constant concentration by evaporation of the alkalis held in solution. Other lost streams fill up after a rainfall, and complete above the ground their course to the main stream. After a heavy rain in the mountains they are apt to change their 'lost' character with a suddenness and decision which may prove dangerous. The water occasionally pours down with an advancing wave or head, which is described as sometimes five or six feet high.

There is one remarkable case in New Mexico where the lost tributaries are plentiful, but the main stream does not exist. This is in a valley which lies between the Rio Grande and the Pecos River. The valley begins near the Sandia Mountains, and is shut out from the streams on each side by broken mountain-chains. It is a well-defined valley, not very broad, but having a length of perhaps three hundred miles. It is somewhat obscured by the small scale, and inaccuracies, of the smaller maps; but on a larger and correct map of the territory its valley-character is unmistakable. It lies much nearer the Rio Grande than the Pecos. Flowing into it, especially on the western side near the upper end, and on the eastern toward the lower, are numerous lost tributaries; but the primary stream has so completely disappeared that its bed can only be found at intervals.

In this valley lie the ruins of the Gran Quivira, the existence of which is not only attested by the ruins themselves, but also by the accounts of the earliest Spanish travellers. The records of the Spanish up to the latter part of the seventeenth century, when they were expelled by the Indians, are incomplete, as the Indians destroyed all that was left behind. That the Gran Quivira was well known to them, however, is shown by the fact that the most prominent ruin there is that of a church. There is now no water for many miles from the ruins. That there must have been once, can well be granted; for no large city would have been built by human beings at a distance of fifteen or twenty miles from a scanty water-supply. The valley may be named from this city, and would then be the Gran Quivira valley.

About half-way down the valley it is broken

by a long, narrow, thin layer of lava, now much broken up, and making a desolate region, locally known as the Mal-pais, or 'bad land.' The crater from which the lava was derived was near the northern end of the Mal-pais. Just above the Mal-pais an old river-bed is reached at the depth of about two hundred and fifty feet: below it, the river-bed, when found, is at a slight depth. South-west of the Apache reservation the old river-bed runs into a large salt-marsh.

A stream of no mean size seems to have once run down this valley. Not only has it now disappeared, but its bed is covered by lava and loose soil sometimes to great depths. As to the cause of the disappearance, it may have some connection with a tradition of the Indians which tells of a year of fire, when this valley was so filled with flames and poisonous gases as to be made uninhabitable. When this occurred, the chronology of the Indians is not perfect enough to tell us. That it was long ago, is attested by the depth to which the old bed is covered by detritus, probably washed down from the mountains, and by trees of considerable size which are found in some places in it. But that it was not so extremely long ago that it had become entirely uninhabitable, is made probable by the comparatively late desertion of the Gran Quivira. It is entirely possible that the Indian year of fire may have long preceded the drying-up of the part of the valley in which Gran Quivira was situated.

M. W. HARRINGTON.

ZUÑIAN CONCEPTIONS OF ANIMAL FORMS AS SHOWN IN POTTERY.

SEVERAL months ago I visited the Pueblo of the Zuñis, and while there enjoyed the opportunity of watching a group of five or six Zuñi women painting some of their pottery.

To show the degree of merit of the Zuñis in their copies of animal forms, one needs no better illustration than their attempts to reproduce the figure of the owl. It is probable that the species of this bird they have used as their model, from time immemorial, is *Bubo virginianus*, the great American horned owl. All the Zuñian clay effigies of owls have horns on their heads; and *Bubo virginianus* is not only the most common owl in the region, but the only one that is thus conspicuously tufted, being characterized by a prominent pair of feather-horns.

My drawing (fig. 1) represents a side view of an adult specimen of the owl in question, with its mandibles intentionally opened, in order to be as much like the Zuñi model (fig. 2) as possible.

This clay copy is the most faithful one I could obtain from a large stock of such material, and one of the best of their attempts in this direction that I have ever met with, or seen figured.

It will be seen that the modeller has represented the tufts upon the head of his subject by a pair of conical elevations. The clay used to make this figure is susceptible of being formed into much more natural-looking tufts than these, yet we never find them. In common with the beak, they are painted a brownish red, in sharp contrast with the white body of the rest of the model. An attempt is always made to represent the feather disk about the eye. Sometimes this is done by two plane concentric circles; other artists make it as shown in fig. 2; and still others have the radiating lines without the limiting-circle. The beak in my specimen is one of the best efforts of the kind that has come under my observation, an attempt evidently having been made to represent its raptorial type. This is not always the case, as may be seen from examining the admirable figures of these models, presented us in Powell's 'Second annual report,' and illustrating Mr. Stevenson's unrivalled collections of 1879.

The body and wings of one of these effigies of the owl come much nearer in form to the body and wings of a young specimen of *Bubo*, say two or three weeks old, than they resemble these parts in the adult owl; the former being short and rounded, and sometimes represented with a tail, and sometimes without. This may have been influenced, originally, from the fact that these young owls are often taken; but they do not acquire the feather-horns until later in life.

We find the talons represented by five characterless points, sometimes radiating as a star, and sometimes arranged with three in front, and one behind, which is better; though these

parts never suggest to us the raptorial foot of the owl.

In criticising one of these Zuñi effigies, we must bear in mind the fact of the great tendency, as in many of the Spanish-American folk, to imitate the work of their ancestors;



FIG. 1.—Right lateral view of head of adult specimen of *Bubo virginianus*; reduced about $\frac{1}{2}$.

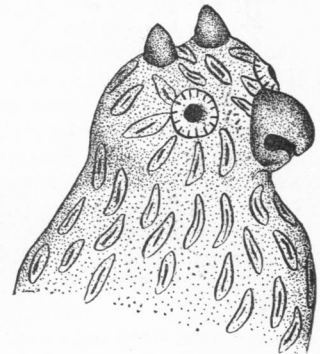


FIG. 2.—Three-quartering view from the right of a Zuñi model of the head of an owl; reduced about $\frac{1}{2}$.

and it would be hard to say for how many generations this clay model of the Zuñian owl has passed down without the slightest attempt at improvement in the direction of a more faithful portraiture of nature. Occasionally, however, an artist will make a lucky hit; and I have two ducks in my possession that illustrate this. The larger and adult one evidently intended to represent a widgeon (*Mareca Americana*); and its likeness, both in coloration and form, is at once quite striking. The other specimen is a young duck of some nondescript variety, the merit of which lies in the rather faithful imitation of the duckling as distinguished from an old bird. This is independent of its size, and, I expect, a difficult effect to successfully produce with the materials at their command, and rarely accomplished.

Their pottery illuminations of birds, as works of art, are no better than could be done by any of our children at eight or nine years of age. Occasionally we find one where the family can be guessed at, but more often the very order is obscure.

Mr. Cushing tells us, in one of his classical contributions to *Century magazine* (May, 1883), about Zuñi, the veneration these people have for the turtle, and how they seem to believe it harbors the soul of some one of their dead, or, as he expresses it, 'our lost others.'

We would naturally expect, therefore, to find

their models of turtles among the best of their clay sculptures. Nor are we disappointed in this, as may be judged from the two drawings (figs. 3 and 4) from a specimen of this kind in my possession.

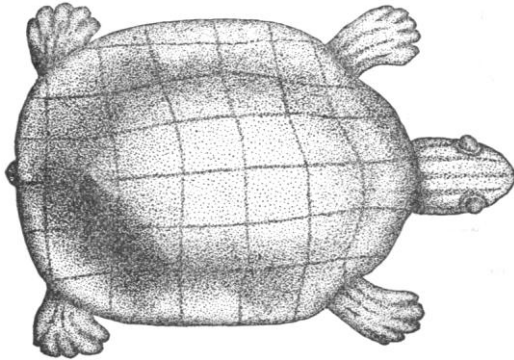


FIG. 3.—Dorsal view of turtle modelled in white clay by Zuñi Indian.

The carapace of this figure is painted a deep brown; while the epidermal plates are simply indicated by six transverse lines, crossed by the same number of longitudinal ones, both in a flesh-red color. This latter tint has also been used to paint the plastron and longitudinal lines on the deep-brown head and feet. This coloration gives it a not distant resemblance to some form of *Chrysemys*. Two such specimens are in my collection; and in both the designer has represented the toes by simply slitting the clay a little ways, in one instance correctly, as seen in the figure; and in the other by three slits, giving each foot only four toes.

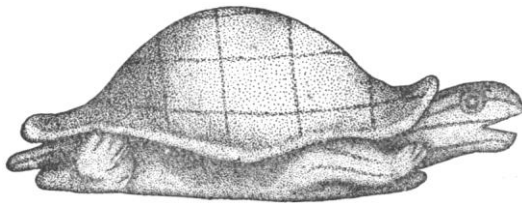


FIG. 4.—The same, lateral aspect. Both less than half the size of original.

I have never seen the turtle depicted upon any of their pottery, and I believe it must be one of their rarer forms to model in clay. So far as I can remember, Mr. Barber does not mention it, or figure the turtle in his article in the *American naturalist*, published some four years ago; nor does Mr. Stevenson allude to it, by word or figure, in the catalogue of his enormous collection of 1879 already quoted.

Mr. Stevenson's figures support another curious fact which I have observed, and will allude to before concluding. It is this: they seem to reserve their amblystomas, their axolotls, their tadpoles, and their bugaboos of human form, to illuminate the quaint clay baskets they manufacture, which usually have handles, and are ornamented with fancy serrated edges, and are of odd shapes. Almost invariably they represent the tadpoles upon side view, and take especial pains to draw the suctorial lips and the eye. The tail, however, is drawn simply by a wriggling line, and is not the broad tail of the tadpole, seen upon lateral aspect of this creature. R. W. SHUFELDT.

TYPES AND THEIR INHERITANCE.

THE object of the anthropologist is plain. He seeks to learn what mankind really are in body and mind, how they came to be what they are, and whither their races are tending; but the methods by which this definite inquiry has to be pursued are extremely diverse. Those of the geologist, the antiquarian, the jurist, the historian, the philologist, the traveller, the artist, and the statistician, are all employed; and the science of man progresses through the help of specialists. Under these circumstances, I think it best to follow an example occasionally set by presidents of sections, by giving a lecture rather than an address, selecting for my subject one that has long been my favorite pursuit, on which I have been working with fresh data during many recent months, and about which I have something new to say.

My data were the family records intrusted to me by persons living in all parts of the country; and I am now glad to think that the publication of some first-fruits of their analysis will show to many careful and intelligent correspondents that their painstaking has not been thrown away. I shall refer to only a part of the work already completed, which in due time will be published; and must be satisfied if, when I have finished this address, some few ideas that lie at the root of heredity shall have been clearly apprehended, and their wide bearings more or less distinctly perceived. I am the more desirous of speaking on heredity, because, judging from private conversations and inquiries that are often put to me, the popular views of what may be expected from inheritance seem neither clear nor just.

The subject of my remarks will be 'Types and their inheritance.' I shall discuss the conditions of the stability and instability of types, and hope, in doing so, to place beyond doubt the existence of a simple and far-reaching law that governs hereditary transmission, and to which I once before ventured to draw

Opening address before the section of anthropology of the British association for the advancement of science, by FRANCIS GALTON, F.R.S., etc., president of the section. From advance sheets of *Nature*.

attention on far more slender evidence than I now possess.

It is some years since I made an extensive series of experiments on the produce of seeds of different size, but of the same species. They yielded results that seemed very noteworthy; and I used them as the basis of a lecture before the Royal institution on Feb. 9, 1877. It appeared from these experiments that the offspring did *not* tend to resemble their parent seeds in size, but to be always more mediocre than they, — to be smaller than the parents, if the parents were large; to be larger than the parents, if the parents were very small. The point of convergence was considerably below the average size of the seeds contained in the large bagful I bought at a nursery-garden, out of which I selected those that were sown.

The experiments showed, further, that the mean filial regression towards mediocrity was directly proportional to the parental deviation from it. This curious result was based on so many plantings, conducted for me by friends living in various parts of the country, — from Nairn in the north, to Cornwall in the south, during one, two, or even three generations of the plants, — that I could entertain no doubt of the truth of my conclusions. The exact ratio of regression remained a little doubtful, owing to variable influences; therefore I did not attempt to define it. After the lecture had been published, it occurred to me that the grounds of my misgivings might be urged as objections to the general conclusions. I did not think them of moment; but as the inquiry had been surrounded with many small difficulties and matters of detail, it would be scarcely possible to give a brief, and yet a full and adequate, answer to such objections. Also, I was then blind to what I now perceive to be the simple explanation of the phenomenon; so I thought it better to say no more upon the subject until I should obtain independent evidence. It was anthropological evidence that I desired, caring only for the seeds as means of throwing light on heredity in man. I tried in vain for a long and weary time to obtain it in sufficient abundance; and my failure was a cogent motive, together with others, in inducing me to make an offer of prizes for family records, which was largely responded to, and furnished me last year with what I wanted. I especially guarded myself against making any allusion to this particular inquiry in my prospectus, lest a bias should be given to the returns. I now can securely contemplate the possibility of the records of height having been frequently drawn up in a careless fashion, because no amount of unbiassed inaccuracy can account for the results, contrasted in their values, but concurrent in their significance, that are derived from comparisons between different groups of the returns.

An analysis of the records fully confirms, and goes far beyond, the conclusions I obtained from the seeds. It gives the numerical value of the regression towards mediocrity as from 1 to $\frac{2}{3}$, with unexpected coherence and precision; and it supplies me with the class of facts I wanted to investigate, — the degrees of family likeness in different degrees of kinship, and the steps

through which special family peculiarities become merged into the typical characteristics of the race at large.

The subject of the inquiry on which I am about to speak was hereditary stature. My data consisted of the heights of 930 adult children, and of their respective parentages, 205 in number. In every case I transmuted the female statures to their corresponding male equivalents, and used them in their transmuted form; so that no objection, grounded on the sexual difference of stature, need be raised when I speak of averages. The factor I used was 1.08, which is equivalent to adding a little less than one-twelfth to each female height. It differs a very little from the factors employed by other anthropologists, who, moreover, differ a trifle between themselves; anyhow it suits my data better than 1.07 or 1.09. The final result is not of a kind to be affected by these minute details; for it happened, that, owing to a mistaken direction, the computer to whom I first intrusted the figures used a somewhat different factor, yet the result came out closely the same.

I shall explain with fulness why I chose stature for the subject of inquiry, because the peculiarities and points to be attended to in the investigation will manifest themselves best by doing so. Many of its advantages are obvious enough, such as the ease and frequency with which its measurement is made, its practical constancy during thirty-five years of middle life, its small dependence on differences of bringing up, and its inconsiderable influence on the rate of mortality. Other advantages which are not equally obvious are no less great. One of these lies in the fact that stature is not a simple element, but a sum of the accumulated lengths or thicknesses of more than a hundred bodily parts, each so distinct from the rest as to have earned a name by which it can be specified. The list of them includes about fifty separate bones, situated in the skull, the spine, the pelvis, the two legs, and the two ankles and feet. The bones in both the lower limbs are counted, because it is the average length of these two limbs that contributes to the general stature. The cartilages interposed between the bones, two at each joint, are rather more numerous than the bones themselves. The fleshy parts of the scalp of the head and of the soles of the feet conclude the list. Account should also be taken of the shape and set of many of the bones which conduce to a more or less arched instep, straight back, or high head. I noticed in the skeleton of O'Brien, the Irish giant, at the College of surgeons, which is, I believe, the tallest skeleton in any museum, that his extraordinary stature of about seven feet seven inches would have been a trifle increased if the faces of his dorsal vertebrae had been more parallel, and his back consequently straighter.

The beautiful regularity in the statures of a population, whenever they are statistically marshalled in the order of their heights, is due to the number of variable elements of which the stature is the sum. The best illustrations I have seen of this regularity were the curves of male and female statures that I obtained from the careful measurements made at

my Anthropometric laboratory in the International health exhibition last year. They were almost perfect.

The multiplicity of elements, some derived from one progenitor, some from another, must be the cause of a fact that has proved very convenient in the course of my inquiry. It is, that the stature of the children depends closely on the average stature of the two parents, and may be considered in practice as having nothing to do with their individual heights. The fact was proved as follows: After transmuting the female measurements in the way already explained, I sorted the children of parents who severally differed 1, 2, 3, 4, and 5 or more inches into separate groups. Each group was then divided into similar classes, showing the number of cases in which the children differed 1, 2, 3, etc., inches from the common average of the children in their respective families. I confined my inquiry to large families of six children and upwards, that the common average of each might be a trustworthy point of reference. The entries in each of the different groups were then seen to run in the same way, except that in the last of them the children showed a faint tendency to fall into two sets, one taking after the tall parent, the other after the short one. Therefore, when dealing with the transmission of stature from parents to children, the average height of the two parents, or, as I prefer to call it, the 'mid-parental' height, is all we need care to know about them.

It must be noted that I used the word parent without specifying the sex. The methods of statistics permit us to employ this abstract term, because the cases of a tall father being married to a short mother are balanced by those of a short father being married to a tall mother. I use the word parent to save a complication due to a fact brought out by these inquiries, that the height of the children of both sexes, but especially that of the daughters, takes after the height of the father more than it does after that of the mother. My parent data are insufficient to determine the ratio satisfactorily.

Another great merit of stature as a subject for inquiries into heredity is, that marriage selection takes little or no account of shortness or tallness. There are undoubtedly sexual preferences for moderate contrast in height; but the marriage choice appears to be guided by so many and more important considerations, that questions of stature exert no perceptible influence upon it. This is by no means my only inquiry into this subject; but, as regards the present data, my test lay in dividing the 205 male parents, and the 205 female parents, each into three groups, — tall, medium, and short (medium being taken as 67 inches and upwards to 70 inches), — and in counting the number of marriages in each possible combination between them. The result was that men and women of contrasted heights, short and tall, or tall and short, married just about as frequently as men and women of similar heights, both tall or both short: there were thirty-two cases of the one to twenty-seven of the other. In applying the law of probabilities to investigations into heredity of stature, we may regard the married

folk as couples picked out of the general population at haphazard.

The advantages of stature as a subject in which the simple laws of heredity may be studied will now be understood. It is a nearly constant value that is frequently measured and recorded; and its discussion is little entangled with considerations of nurture, of the survival of the fittest, or of marriage selection. We have only to consider the mid-parentage, and not to trouble ourselves about the parents separately. The statistical variations of stature are extremely regular; so much so, that their general conformity with the results of calculations, based on the abstract law of frequency of error, is an accepted fact by anthropologists. I have made much use of the properties of that law in cross-testing my various conclusions, and always with success.

The only drawback to the use of stature is its small variability. One-half of the population with whom I dealt varied less than 1.7 inches from the average of all of them; and one-half of the offspring of similar mid-parentages varied less than 1.5 inches from the average of their own heights. On the other hand, the precision of my data is so small, partly due to the uncertainty in many cases whether the height was measured with the shoes on or off, that I find by means of an independent inquiry, that each observation, taking one with another, is liable to an error that as often as not exceeds two-thirds of an inch.

It must be clearly understood, that my inquiry is primarily into the inheritance of different degrees of tallness and shortness; that is to say, of measurements made from the crown of the head to the level of mediocrity, upwards or downwards as the case may be, and not from the crown of the head to the ground. In the population with which I deal, the level of mediocrity is 68½ inches (without shoes). The same law applying with sufficient closeness both to tallness and shortness, we may include both under the single head of deviations; and I shall call any particular deviation a 'deviate.' By the use of this word, and that of 'mid-parentage,' we can define the law of regression very briefly. It is, that the height-deviate of the offspring is, on the average, two-thirds of the height-deviate of its mid-parentage.

If this remarkable law had been based only on experiments on the diameters of the seeds, it might well be distrusted until confirmed by other inquiries. If it were corroborated merely by the observations on human stature, of which I am about to speak, some hesitation might be expected before its truth could be recognized in opposition to the current belief that the child tends to resemble its parents. But more can be urged than this. It is easily to be shown that we ought to expect filial regression, and that it should amount to some constant fractional part of the value of the mid-parental deviation. It is because this explanation confirms the previous observations made both on seeds and on men, that I feel justified on the present occasion in drawing attention to this elementary law.

The explanation of it is as follows: The child inherits partly from his parents, partly from his ances-

try. Speaking generally, the farther his genealogy goes back, the more numerous and varied will his ancestry become, until they cease to differ from any equally numerous sample taken at haphazard from the race at large. Their mean stature will then be the same as that of the race; in other words, it will be mediocre. Or, to put the same fact into another form, the most probable value of the mid-ancestral deviates in any remote generation is zero.

For the moment let us confine our attention to the remote ancestry, and to the mid-parentages, and ignore the intermediate generations. The combination of the zero of the ancestry with the deviate of the mid-parentage, is that of nothing with something; and the result resembles that of pouring a uniform proportion of pure water into a vessel of wine. It dilutes the wine to a constant fraction of its original alcoholic strength, whatever that strength may have been.

The intermediate generations will, each in its degree, do the same. The mid-deviate of any one of them will have a value intermediate between that of the mid-parentage and the zero value of the ancestry. Its combination with the mid-parental deviate will be as if not pure water, but a mixture of wine and water in some definite proportion, had been poured into the wine. The process throughout is one of proportionate dilutions, and therefore the joint effect of all of them is to weaken the original wine in a constant ratio.

We have no word to express the form of that ideal and composite progenitor, whom the offspring of similar mid-parentages most nearly resemble, and from whose stature their own respective heights diverge evenly, above and below. He, she, or it, may be styled the 'generant' of the group. I shall shortly explain what my notion of a generant is, but for the moment it is sufficient to show that the parents are not identical with the generant of their own offspring.

The average regression of the offspring to a constant fraction of their respective mid-parental deviations, which was first observed in the diameters of seeds, and then confirmed by observations on human stature, is now shown to be a perfectly reasonable law which might have been deductively foreseen. It is of so simple a character, that I have made an arrangement with one movable pulley, and two fixed ones, by which the probable average height of the children of known parents can be mechanically reckoned. This law tells heavily against the full hereditary transmission of any rare and valuable gift, as only a few of many children would resemble their mid-parentage. The more exceptional the gift, the more exceptional will be the good fortune of a parent who has a son who equals him, and still more if he has a son who overpasses him. The law is even-handed: it levies the same heavy succession-tax on the transmission of badness as well as of goodness. It discourages the extravagant expectations of gifted parents that their children will inherit all their powers, it no less discountenances extravagant fears that they will inherit all their weaknesses and diseases.

The converse of this law is very far from being its numerical opposite. Because the most probable deviate of the son is only two-thirds that of his mid-parentage, it does not in the least follow that the most probable deviate of the mid-parentage is $\frac{3}{2}$, or $1\frac{1}{2}$ that of the son. The number of individuals in a population who differ little from mediocrity is so preponderant, that it is more frequently the case that an exceptional man is the somewhat exceptional son of rather mediocre parents, than the average son of very exceptional parents. It appears from the very same table of observations by which the value of the filial regression was determined, when it is read in a different way, namely, in vertical columns instead of in horizontal lines, that the most probable mid-parentage of a man is one that deviates only one-third as much as the man does. There is a great difference between this value of $\frac{1}{3}$, and the numerical converse mentioned above of $\frac{3}{2}$; it is four and a half times smaller, since $4\frac{1}{2}$, or $\frac{9}{2}$, being multiplied into $\frac{1}{3}$, is equal to $\frac{3}{2}$.

Let it not be supposed for a moment, that these figures invalidate the general doctrine that the children of a gifted pair are much more likely to be gifted than the children of a mediocre pair. What it asserts is, that the ablest child of one gifted pair is not likely to be as gifted as the ablest of all the children of very many mediocre pairs. However, as, notwithstanding this explanation, some suspicion may remain of a paradox lurking in these strongly contrasted results, I will explain the form in which the table of data was drawn up, and give an anecdote connected with it. Its outline was constructed by ruling a sheet into squares, and writing a series of heights in inches, such as 60 and under 61, 61 and under 62, etc., along its top, and another similar series down its side. The former referred to the height of offspring, the latter to that of mid-parentages. Each square in the table was formed by the intersection of a vertical column with a horizontal one; and in each square was inserted the number of children out of the 930 who were of the height indicated by the heading of the vertical column, and who, at the same time, were born of mid-parentages of the height indicated at the side of the horizontal column. I take an entry out of the table as an example. In the square where the vertical column headed '69' is intersected by the horizontal column by whose side '67' is marked, the entry 38 is found; this means, that, out of the 930 children, 38 were born of mid-parentages of 69 and under 70 inches, who also were 67 and under 68 inches in height. I found it hard at first to catch the full significance of the entries in the table, which had curious relations that were very interesting to investigate. Lines drawn through entries of the same value formed a series of concentric and similar ellipses. Their common centre lay at the intersection of the vertical and horizontal lines that corresponded to 68 $\frac{1}{2}$ inches. Their axes were similarly inclined. The points where each ellipse in succession

¹ A matter of detail is here ignored which has nothing to do with the main principle, and would only serve to perplex if I described it.

was touched by a horizontal tangent, lay in a straight line inclined to the vertical in the ratio of $\frac{3}{5}$; those where they were touched by a vertical tangent, lay in a straight line inclined to the horizontal in the ratio of $\frac{1}{3}$. These ratios confirm the values of average regression already obtained by a different method, of $\frac{3}{5}$ from mid-parent to offspring, and of $\frac{1}{3}$ from offspring to mid-parent. These and other relations were evidently a subject for mathematical analysis and verification. They were all clearly dependent on three elementary data, supposing the law of frequency of error to be applicable throughout; these data being 1°, the measure of racial variability; 2°, that of co-family variability (counting the offspring of like mid-parentages as members of the same co-family); and, 3°, the average ratio of regression. I noted these values, and phrased the problem in abstract terms such as a competent mathematician could deal with, disentangled from all reference to heredity, and in that shape submitted it to Mr. J. Hamilton Dickson, of St. Peter's college, Cambridge. I asked him kindly to investigate for me the surface of frequency of error that would result from these three data, and the various particulars of its sections, one of which would form the ellipses to which I have alluded.

I may be permitted to say that I never felt such a glow of loyalty and respect towards the sovereignty and magnificent sway of mathematical analysis as when his answer reached me, confirming, by purely mathematical reasoning, my various and laborious statistical conclusions with far more minuteness than I had dared to hope; for the original data ran somewhat roughly, and I had to smooth them with tender caution. His calculation corrected my observed value of mid-parental regression from $\frac{1}{3}$ to $\frac{6}{17.6}$:

the relation between the major and minor axis of the ellipses was changed 3 per cent, their inclination was changed less than 2°. It is obvious, then, that the law of error holds throughout the investigation with sufficient precision to be of real service, and that the various results of my statistics are not casual determinations, but strictly interdependent.

In the lecture at the Royal institution to which I have referred, I pointed out the remarkable way in which one generation was succeeded by another that proved to be its statistical counterpart. I there had to discuss the various agencies of the survival of the fittest, of relative fertility, and so forth; but the selection of human stature as the subject of investigation now enables me to get rid of all these complications, and to discuss this very curious question under its simplest form. How is it, I ask, that in each successive generation, there proves to be the same number of men per thousand who range between any limits of stature we please to specify, although the tall men are rarely descended from equally tall parents, or the short men from equally short? How is the balance from other sources so nicely made up? The answer is, that the process comprises two opposite sets of actions, one concentrative and the other dispersive, and of such a char-

acter that they necessarily neutralize one another, and fall into a state of stable equilibrium. By the first set, a system of scattered elements is replaced by another system which is less scattered; by the second set, each of these new elements becomes a centre, whence a third system of elements is dispersed. The details are as follows: In the first of these two stages, the units of the population group themselves, as it were by chance, into married couples, whence the mid-parentages are derived; and then by a regression of the values of the mid-parentages the true generants are derived. In the second stage, each generant is a centre whence the offspring diverge. The stability of the balance between the opposed tendencies is due to the regression being proportionate to the deviation,—it acts like a spring against a weight.

A simple equation connects the three data of race variability, of the ratio of regression, and of co-family variability; whence, if any two are given, the third may be found. My observations give separate measures of all three, and their values fit well into the equation, which is of the simple form, —

$$v^2 \frac{p^2}{2} + f^2 = p^2,$$

where $v = \frac{3}{5}$, $p = 1.7$, $f = 1.5$.

It will therefore be understood that a complete table of mid-parental and filial heights may be calculated from two simple numbers.

It will be gathered from what has been said, that a mid-parental deviate of one unit implies a mid-grandparental deviate of $\frac{1}{3}$, a mid-ancestral unit in the next generation of $\frac{1}{9}$, and so on. I reckon from these and other data, by methods that I cannot stop to explain, that the heritage derived on an average from the mid-parental deviate, independently of what it may imply, or of what may be known concerning the previous ancestry, is only $\frac{1}{2}$. Consequently, that similarly derived from a single parent is only $\frac{1}{4}$, and that from a single grandparent is only $\frac{1}{16}$.

The most elementary data upon which a complete table of mid-parental and filial heights admits of being constructed, are, 1°, the ratio between the mid-parental and the rest of the ancestral influences; and, 2°, the measure of the co-family variability.

I cannot now pursue the numerous branches that spring from the data I have given, as from a root. I will not speak of the continued domination of one type over others, or of the persistency of unimportant characteristics, or of the inheritance of disease, which is complicated in many cases by the requisite concurrence of two separate heritages, the one of a susceptible constitution, the other of the germs of the disease. Still less can I enter upon the subject of fraternal characteristics, which I have also worked out. It will suffice for the present to have shown some of the more important conditions associated with the idea of race, and how the vague word 'type' may be defined by peculiarities in hereditary transmission; at all events, when that word is applied to any single quality, such as stature. To include those

numerous qualities that are not strictly measurable, we must omit reference to number and proportion, and frame the definition thus: 'The type is an ideal form towards which the children of those who deviate from it tend to regress.'

The stability of a type would, I presume, be measured by the strength of its tendency to regress; thus a mean regression from 1 in the mid-parents to $\frac{2}{3}$ in the offspring would indicate only half as much stability as if it had been to $\frac{1}{3}$.

The mean regression in stature of a population is easily ascertained, but I do not see much use in knowing it. It has already been stated that half the population vary less than 1.7 inches from mediocrity, this being what is technically known as the 'probable' deviation. The mean deviation is, by a well-known theory, 1.18 times that of the probable deviation, therefore in this case it is 1.9 inches. The mean loss through regression is $\frac{1}{3}$ of that amount, or a little more than .6 inch. That is to say, taking one child with another, the mean amount by which they fall short of their mid-parental peculiarity of stature is rather more than six-tenths of an inch.

With respect to these and the other numerical estimates, I wish emphatically to say, that I offer them only as being serviceably approximate, though they are mutually consistent; and with the desire that they may be reinvestigated by the help of more abundant and much more accurate measurements than those I have had at command. There are many simple and interesting relations to which I am still unable to assign numerical values for lack of adequate material, such as that to which I referred some time back, of the superior influence of the father over the mother on the stature of their sons and daughters.

The limits of deviation beyond which there is no regression, but a new condition of equilibrium is entered into, and a new type comes into existence, have still to be explored. Let us consider how much we can infer from undisputed facts of heredity regarding the conditions amid which any form of stable equilibrium, such as is implied by the word 'type,' must be established, or might be disestablished and superseded by another. In doing so I will follow cautiously along the same path by which Darwin started to construct his provisional theory of pangenesis; but it is not in the least necessary to go so far as that theory, or to entangle ourselves in any questioned hypothesis.

There can be no doubt that heredity proceeds to a considerable extent, perhaps principally, in a piecemeal or piebald fashion, causing the person of the child to be to that extent a mosaic of independent ancestral heritages, one part coming with more or less variation from this progenitor, and another from that. To express this aspect of inheritance, where particle proceeds from particle, we may conveniently describe it as 'particulate.'

So far as the transmission of any feature may be regarded as an example of particular inheritance, so far (it seems little more than a truism to assert) the element from which that feature was developed must

have been particulate also. Therefore, wherever a feature in a child was not personally possessed by either parent, but transmitted through one of them from a more distant progenitor, the element whence that feature was developed must have existed in a particulate, though impersonal and latent form, in the body of the parent. The total heritage of that parent will have included a greater variety of material than was utilized in the formation of his own personal structure. Only a portion of it became developed: the survival of at least a small part of the remainder is proved, and that of a larger part may be inferred by his transmitting it to the person of his child. Therefore the organized structure of each individual should be viewed as the fulfilment of only one out of an indefinite number of mutually exclusive possibilities. It is the development of a single sample drawn out of a group of elements. The conditions under which each element in the sample became selected are, of course, unknown; but it is reasonable to expect they would fall under one or other of the following agencies: first, self-selection, where each element selects its most suitable neighbor, as in the theory of pangenesis; secondly, general co-ordination, or the influence exerted on each element by many or all of the remaining ones, whether in its immediate neighborhood or not; finally, a group of diverse agencies, alike only in the fact that they are not uniformly helpful or harmful, that they influence with no constant purpose: in philosophical language, that they are not teleological; in popular language, that they are accidents or chances. Their inclusion renders it impossible to predict the peculiarities of individual children, though it does not prevent the prediction of average results. We now see something of the general character of the conditions amid which the stable equilibrium that characterizes each race must subsist.

Political analogies of stability and change of type abound, and are useful to fix the ideas, as I pointed out some years ago. Let us take that which is afforded by the government of a colony which has become independent. The individual colonists rank as particulate representatives of families or other groups in the parent country. The organized colonial government ranks as the personality of the colony, being its mouthpiece and executive. The government is evolved amid political strife, one element prevailing here, and another there. The prominent victors band themselves into the nucleus of a party: additions to their number, and revisions of it, ensue, until a body of men are associated capable of conducting a completely organized administration. The kinship between the form of government of the colony and that of the parent state is far from direct, and resembles in a general way that which I conceive to subsist between the child and his mid-parentage. We should expect to find many points of resemblance between the two, and many instances of great dissimilarity; for our political analogy teaches us only too well on what slight accidents the character of the government may depend when parties are nearly balanced.

The appearance of a new and useful family peculiarity is a boon to breeders, who by selection in mating gradually reduce the preponderance of those ancestral elements that endanger reversion. The appearance of a new type is due to causes that lie beyond our reach; so we ought to welcome every useful one as a happy chance, and do our best to domicile and perpetuate it. When heredity shall have become much better, and more generally understood than now, I can believe that we shall look upon a neglect to conserve any valuable form of family type as a wrongful waste of opportunity. The appearance of each new natural peculiarity is a faltering step in the upward journey of evolution, over which, in outward appearance, the whole living world is blindly blundering and stumbling, but whose general direction man has the intelligence dimly to discern, and whose progress he has power to facilitate.

A NEW THEORY OF COHESION.

SINCE a great part of the relations discussed in a paper by Dr. H. Whiting, on a new theory of cohesion (*Proc. Amer. acad.*, xix. 353), are determined by the equation between the pressure, volume, and temperature of a given quantity of the substance considered, a comparison of the form of this equation as given in this paper with forms previously proposed affords the readiest means of comparing the author's results with those of previous investigators. The equation proposed in 1873 by Van der Waals has the form

$$\left(p + \frac{a}{v^2}\right)\left(1 - \frac{b}{v}\right)v = Rt; \quad (1)$$

that of the present paper (see p. 376, third equation) may be written

$$\left(p + \frac{a}{v^2}\right)\left(1 - \sqrt[3]{\frac{c}{v}}\right)v = Rt. \quad (2)$$

In both equations, p , v , and t denote pressure, volume, and absolute temperature: the other letters denote constants, to be determined by the nature of the substance considered.

We may get some idea of the numerical difference in the indications of these equations, if we observe that the ratio of the volume of the critical state to that which would be required by the laws of Boyle and Charles is 0.375 by the first equation, and 0.536 by the second (the experiments of Dr. Andrews give something like 0.414 for carbonic acid). Again: the ratio of the volume of the critical state to that at absolute zero would be 3 by the first equation (which, however, was not intended to apply to such a determination), and 3.58 by the second.

The equation of Dr. Whiting has an important property in common with that of Van der Waals. If we use the pressure, volume, and temperature of the critical state as units for the measurement of the pressure, volume, and temperature of all states, the constants will disappear from either equation, and we obtain a relation between the pressure, volume, and temperature (thus measured), which should be the same for all bodies. From this property of his equation, independently of the particular relation

obtained, Van der Waals has derived a considerable number of interesting conclusions, which would equally follow from the equation of Dr. Whiting (see the twelfth and thirteenth chapters of the German translation of the memoirs of the former, by Dr. Roth, Leipzig, 1881). One of these is mentioned in Dr. Whiting's treatise, p. 427.

It is well known that the equation of Van der Waals agrees with experiment to an extent which is quite remarkable when the simplicity of the equation is considered, and the complexity of the problem to which it relates. But it was not intended to be applied to states as dense as the ordinary liquid state. Dr. Whiting's equation, on the other hand, seems to have been formed with especial reference to the denser conditions of matter, and, from the numerical verifications which are given, would appear to represent the ordinary liquid state, in some respects at least, much better than the equation of Van der Waals. The principal verifications relate to the coefficient of expansion and the critical temperature. When the pressure may be neglected, as in the ordinary liquid state, equation (2) gives

$$\frac{de}{dt} = \frac{7}{3}e^2 + \frac{4}{3}te^3,$$

where e is the coefficient of expansion $\left(\frac{dv}{vdt}\right)$. A very elaborate comparison is made between this equation and the experiments of Kopp, Pierre, and Thorpe. An empirical formula of Dr. Mendeleeff is also considered, which gives

$$\frac{de}{dt} = e^2,$$

a value of de/dt about one-third as great as Dr. Whiting's. We may add that the equation (1) of Van der Waals would give

$$\frac{de}{dt} = 3e^2 + 2te^3,$$

a value of de/dt about one-third greater than Dr. Whiting's. The result seems to be that the indications of experiment lie between the formulae of Dr. Whiting and Dr. Mendeleeff (pp. 424 ff.). We may conclude that they would not agree so well with that of Van der Waals.

Each of the equations (1) and (2) will give the critical temperature when we know the coefficient of expansion for a given temperature. Dr. Whiting has calculated the critical temperature, by means of his equation, for twenty-six substances for which this temperature has been observed. The calculated and observed values generally differ by less than ten degrees Centigrade. An equation derived by Thorpe and Rücker, in part from the formula of Mendeleeff above mentioned, and in part from a principle of Van der Waals, gives about the same agreement with experiment. We may add that the general equation of Van der Waals, taken alone, would give for the critical temperature t_c the formula

$$t_c = \frac{8(2te + 1)^2}{27e(te + 1)},$$

which does not seem, from the test of a few cases, to agree so well with experiment.

In establishing his fundamental equations, Dr. Whiting, like Van der Waals, treats the molecules as elastic spheres which attract one another when not in contact. The cohesive effect of the molecular attraction is regarded by both as proportional to the square of the density. It is, in fact, represented by the same term $\left(\frac{a}{v^2}\right)$ in equations (1) and (2). This effect is deduced by Dr. Whiting from the hypothesis of a molecular attraction varying inversely as the fourth power of the distance, by supposing a body to expand so that every distance is increased in the same ratio; but such an expansion is entirely unlike any which actually occur in fluids, since it increases the distance within which the centres of molecules do not approach one another. We shall probably come much nearer to the case of nature, if we suppose that the average number of molecules in a fluid, which are between the distances r and $r + dr$ from a given molecule, varies as the density of the fluid. This supposition will evidently make the cohesive effect of the molecular attraction vary as the square of the density. It would seem that any agreement of experiment with the indications either of equation (1) or of equation (2) should be regarded as confirmatory of this law of the distribution of the molecules rather than of any particular law of attraction.

THURSTON'S FRICTION AND LOST WORK.

THIS volume combines characteristics not too often found in a work on this or kindred subjects. It is thoroughly scientific in method, as well as in the treatment of separate problems. It is eminently practical in results, as well as in the selection and range of the problems considered. It is clear, accurate, and minute in the details which give completeness to its discussions, and make them readily available for actual use. It is not merely or principally a compilation. While it brings together the formulae and results of the standard writers and experimenters upon friction, its laws, modifications, and effects, it also includes the author's own elaborate experiments, made with a view to their bearing upon questions of daily and vital importance to the engineer and the student. The conclusions drawn from these experiments, being always subject to comparison with the facts and knowledge gained by the author in a wide and extensive engineering practice, are rational and reliable. The book comprises eight chapters. The first explains the object of mechanism, the manner of computing work and power, the laws of the per-

sistence and transformation of energy, and the relation of lost work to the efficiency of mechanism. In the second chapter, the theory and laws of friction are developed. The problems which arise in practice are taken up one by one, clearly analyzed, mathematically solved, and the applications of the resulting formulae pointed out.

The next three chapters form an exhaustive treatise on the lubricants used for reducing friction; their nature and relative values; the means of applying and using; methods of analyzing, inspecting, and testing them. Cuts of the best lubricators in use, and also of the apparatus used in making physical tests; tables giving physical and chemical properties of oils, their color reactions, density, specific gravity, and viscosity; and diagrams showing the relations of viscosity and lubrication, and effects of temperature upon viscosity, accompany the text. Oleography and electrical conductivity are noticed as methods of identifying various oils. The nature and effects of friction, and the kinds and properties of lubricants, having been thus fully discussed, the author proceeds to the subject of experiments, from which must be obtained the values of constants which enter into all the formulae. Upon the correctness of these values depends the accuracy of results obtained by calculation from the formulae developed by the theoretical investigations.

The sixth chapter relates to experiments of two kinds: First, those designed to ascertain the relative amounts of friction between different surfaces under varying conditions; to determine constants, or suggest the value and form of empirical formulae, applicable to friction of both solids and fluids. Second, experiments with machines for testing lubricants, with cuts and descriptions of oil-testing machines. The mathematical theory and method of using Thurston's machine are given in detail, together with tables showing records of oil-tests made by the author with his machine. The seventh chapter gives results of experiments with lubricants, showing their effects in modifying friction; their endurance under different conditions of pressure and velocity; and the effect of changes of pressure and velocity upon the coefficient of friction.

It is impossible to give in a brief review an adequate idea of the minuteness of detail with which the wide range of problems and experiments are discussed. The reader may expect to find, substantially, all that is known upon these subjects through the investigations of earlier writers, supplemented by the results of

the author's own work in his professional practice, and in carefully conducted experiments.

A somewhat novel feature of the book, and one which will commend it to the manufacturer and mill-owner, is the closing chapter on 'The finance of lost work.' The lost work, the cost of the lubricant, the quantity used, and the saving or loss of energy effected by the change of one lubricant for a better or a poorer, are represented by symbols, and embodied in equations, by which general principles, as well as special results, are reached. The application of these equations is illustrated by the solution of several problems.

While the author points out the need of more extended experiments in some directions, and warns the reader against drawing conclusions too hastily from insufficient data, it would seem that the method outlined, and so extensively pursued, covers the whole ground of investigation required for the complete solution of all questions relating to losses due to friction in mechanism.

DARWIN'S BIOGRAPHY.

SOME men are great, and some men famous: a few are both, and among them Darwin is pre-eminent. Greatness is a quality, fame a circumstance, which greatness, unhelpt by fortune, cannot secure. In this century, there have been many great intellects celebrated to the votaries of science for their achievements, yet not famous with the public. Darwin is not solitarily pre-eminent: of his own generation we may count a number his compeers. He stands high aloft, yet he is even overtopped in sheer greatness by his greatest contemporaries; but, among them all, not one equals Darwin in deserved fame. The influential importance of a discovery is measured neither by the ability of the discoverer, nor by the magnitude of the difficulties overcome. There have been other intellectual efforts as successful and grand in their making and results, as that which established the Darwinian theory, but, in our time, no other of equally profound far-reaching and lasting significance to mankind.

In studying Darwin we have to bear in mind to separate the greatness of the man from the fame of his influence. The time has not yet

come to fully estimate the man — we must await the biography promised by the family; but we are already able to appreciate the directness and force of his intellect, his noble candor, his courage under suffering, and, above all, his insatiable love of knowledge and research; we can appreciate also the revolution of belief he caused.

If a poet were to imagine forth a career, which without adventurous incident, or participation in the great affairs of nations, should stir the world, he might, if a great artist, conceive a character at once simple and noble; endowed with irrepressible love of knowledge; given over to study; indefatigable in gathering facts, and always marshalling them into logical phalanxes, making the front and flanks of his evidence alike impregnable: he would place this character aside from the bustle of the world, and perhaps add ill health to the conditions to enforce closer retirement, and accentuate the obscurity of secluded labor; and the poet-artist would endow his created man with means, that his life might be altogether devoted to study, without pecuniary harassments to impede the absolute concentration of mental effort. Last of all, the poet's conception includes a great idea. For year after year the toil would continue, unheeded but prosperous, until the long-growing thought becomes a proven generalization — the whilom mystery of nature is clarified. At last the result is given to the world; it turns the minds of men topsy-turvy; all civilized nations are convulsed with the turmoil of discussion, angry and turbulent: the suddenly famous philosopher maintains his retirement; he withholds himself from all share in the profitless fury he has aroused; he does not swerve from the continuation of his unobtrusive labors, but repeatedly re-enforces by more facts and more logic his published generalization. In a few years the vituperation, which was hurled at him in unmeasurable quantity, ceases; yet a little while, and his due is paid — fully: the world, that had but just now reviled him, turns about and acknowledges the mighty progress the one man has accomplished for all. Now the work is finished: the rich recompense of universal gratitude has been earned and received. Then the life closes, honored by every class and in every country. At his death it is already known that this student's name will henceforth mark the century in which he lived, because the kings, generals, and politicians of his time are all less than the unpretentious investigator.

Our supposed poet, the maker of this his-

Charles Darwin und sein verhältniss zu Deutschland. Von Dr. ERNST KRAUSE. Mit zahlreichen bisher ungedruckten briefen Darwins, zwei portraits, handschriftprobe, u.s.w., m. lichtdruck. Leipzig, Günther, 1885. 8+236 p. 8".

tory, would have given us one of the noblest creations of genius, — one full of inspiration to every master and drudge of science. But the poet is too late: the career he should invent has been lived in reality by Darwin. Can any one contemplate it, and not feel that it is beautiful?

The biography of Darwin is a theme worthy of genius: it should be written with eloquence, as well as with insight and discrimination. But Darwin's life possesses so much inherent interest, that any conscientious narrative of it must be meritorious. Dr. Krause furnishes us with a biography the preliminary character of which is frankly confessed. It is not in any sense the great work we hope for, nevertheless it deserves genuine praise. The author gives a vivid and loving sketch of Darwin's career, and adds enough of the personal history to convey a clear impression of his character, which was so pure and open, that its noble traits impressed not only his friends, but also all who knew him. Indeed, there are many who feel that the man was finer than any of his works. Dr. Krause, as was natural for the editor of the German evolutionary magazine, *Kosmos*, has made his book more than a personal history by including an account of the rise and triumph of the Darwinian theory. All this is so well done, that the book affords a very clear idea of the inception of the theory, and of the leading episodes of the prolonged warfare which was initiated by the publication of the 'Origin of species.' It is certainly a very interesting history, plainly but well told. Moreover the volume, albeit not large, contains a sufficient outline of all Darwin's chief investigations. The principal excellence of the work, however, lies in the correlations it establishes between Darwin's labors and both the circumstances of his life and his personal traits. In short, we commend the book as the best available source of a general knowledge of Darwin. The volume gains in interest by a couple of fairly good portraits — a view of Darwin's home at Down, a *facsimile* of an autograph letter — and the publication of a not inconsiderable number of letters from Darwin to various German naturalists. It is well printed in clear Roman type, not in Gothic abominations. It may be noted, that it is to be followed by a companion volume of translations into German of such of Darwin's smaller writings as have not previously appeared in that language. We hope that this biography will be soon published in English translation.

We have endeavored to express the twofold

nature of the interest Darwin excites. Dr. Krause portrays his greatness, but his fame must be explained more fully hereafter by some profound philosopher who knows thoroughly and understandingly the intellectual history of the nineteenth century.

EDIBLE AND POISONOUS FUNGI.

ALTHOUGH the larger fungi, popularly called toadstools and mushrooms, are not so dangerous as is generally believed, it is certainly a difficult matter for the public to distinguish between the forms which are edible, and those which are injurious, or even fatal. The two charts, with twelve colored plates by Prang, are intended to aid those who are not botanical experts, in recognizing some of our more common edible and poisonous species. With each plate is a brief description of the species figured, and directions for cooking; and, under the heading of 'general directions,' Mr. Julius A. Palmer gives a short account of the distinctions between poisonous and edible fungi. The plates are, in general, well executed and characteristic; and some of the best edible species, as *Coprinus comatus*, would be recognized, without hesitation, by the most inexperienced. The plate showing puffballs is not well done from a botanical point of view; and, with regard to the plates in which several different species are shown in one group, it may be said that the effect is confusing; especially in the plate of *Russulæ*, where, after the directions for cooking, the warning is added, "the noxious members of this family resemble the esculent so closely, that, to the amateur, tasting each one as gathered is the only guide; the hurtful ones being always hot and acrid." In such a case, one would suppose that plates would be of little use to the general public. In continuations of this work, it is to be hoped that the crowding of several species on one plate will be abandoned.

If Americans do not make use of fungi to the same extent as some other nations, it is, perhaps, quite as much owing to their ignorance of the way to cook them, as fear of mistaking the edible and noxious forms. Numbers of our common species are delicious when well cooked: but on the other hand, as usually prepared for the table, they are quite the reverse; and, until the number of good cooks is much greater than it is now, we can hardly expect fungi to become a very popular

Mushrooms of America, edible and poisonous. By JULIUS A. PALMER, JUN. Boston, Prang, 1885.

article of food. The mycophagists of the country are not as yet numerous; but they sometimes do an injury to their cause, by recommending the use of certain species of which perhaps the best that can be said is that they are not injurious. *Agaricus procerus*, and *Boletus strobilaceus*, figured in the present work, would not strictly be called edible, except by an enthusiastic mycophagist. We imagine that one whose first experiment in fungus-eating was made upon either of the species just named, would hardly be likely to repeat the experiment.

ROHÉ'S HYGIENE.

THIS book, of small size and modest appearance, is full of important matter, told in a very interesting manner. The preface says it is intended as a guide to the principles and practice of preventive medicine; and we think that every student of medicine should possess it, and study it. Air, water, food, clothing, soil, dwellings, hospitals, camp-life, and numerous other every-day topics, are discussed in condensed sections, but with clearness and intelligence. There are some points, however, which we think should receive greater attention. For instance, in giving the tests for air and water impurities, nothing is said of the methods of analyzing these media for germs. A short paragraph states that the air is the bearer of germs, and that quantitative analyses of the same have recently been made. Although the methods of such analyses are elaborate, and too expensive for students in general to undertake, nevertheless they ought to be explained in a text-book of this kind.

Emphasis is properly laid upon the dangers from sewage in drinking-water. Dr. Rohé takes exceptions to the statement, that rivers quickly purify themselves; and he quotes the report of the Massachusetts board of health for 1876, in which the foul condition of the Blackstone River was proven. He rightly claims that the rate of self-purification for rivers is limited, and may be easily exceeded by the rate of sewage pollution. The danger from using polluted ice is also described and illustrated by reference to cases of disease caused by such ice. Water does not purge itself of impurities by freezing.

The proof-reading of the book seems to have been hastily done, as we notice numerous errors of spelling. We heartily recommend the book, and praise it for the sincere and unaffected spirit in which it is written.

A text-book of hygiene. By GEORGE H. ROHÉ, M.D. Baltimore, Thomas & Evans, 1885. 8°.

THE RESCUE OF GREELY.

IN welcoming Lieut. Greely to the meeting of the geographical section of the British association last summer, Capt. Bedford Pim, himself an arctic traveller of great experience, said that on one of the early expeditions in search of Sir John Franklin, the American ships were observed dashing into the ice ahead of their English companions. "Yes," said an old quartermaster: "they fears nothing, because they knows nothing." But now, since the return of Greely, the gallant captain added, it was evident that "the Americans knew every thing, and feared nothing." This, too, must be the verdict of every one who reads this book, and sees the way in which Schley and Emory, in two Dundee whalers, not merely kept pace with the best ships in the Dundee whaling-fleet, but, pushing by them, rescued Greely and his dying comrades hours, if not days (considering the uncertainties of ice navigation), before the other ships could have reached Cape Sabine, thus saving the lives of several of the party.

It makes an interesting story, and is well told by Professor Soley, who, we suppose, wrote the greater part, if not all, of the narrative. The introductory chapters on the gateway of the polar seas and the circumpolar stations, are too brief to be of much value; while the account of the two previous attempts to reach Greely contains little that will aid one in forming for himself an opinion as to where the responsibility for the deaths of nineteen out of the twenty-five members of the Lady Franklin Bay expedition really belongs. The volume further contains a few good pictures; a track-chart showing the route of Schley's vessels; and the official chart of the region from Baffin Bay to Lincoln Sea, first published in *Science* last February.

NOTES AND NEWS.

A LETTER from Dr. Willis Everette, U.S.A., who recently arrived from St. Michaels, Alaska, at San Francisco, states that his original plan of crossing from the headwaters of the White-river branch of the Yukon to the Copper River, was defeated by the impossibility of getting any companion, either white or native, to undertake the voyage with him. Being thus unaccompanied, he was incommoded by the behavior of the Upper Yukon Indians, who endeavored to purloin his supplies; and therefore he descended

The rescue of Greely. By W. S. SCHLEY and J. R. SOLEY. Illustrated from the photographs and maps of the relief expedition. New York, Charles Scribner's Sons, 1885.

the river in August and September, 1884, to the region of the American trading-posts. Here he remained until the present summer, returning on the steamer *St. Paul*, to San Francisco, Aug. 30. Dr. Everette has occupied himself in collecting geographical data from traders, natives, and explorers; making sketches of trading-posts, native villages, and other points of interest on the Yukon; bringing together facts in relation to the fauna, flora, and ethnology of the Yukon and adjacent rivers; obtaining data for a history of the explorations by Americans since the purchase of Alaska, and collecting a full series of vocabularies from the Yukon tribes. He has particularly interesting geographical information from the little known Yukon Delta, the Tananah and Upper Kuskokwim rivers, the Shageluk district, and various hitherto imperfectly explored affluents of the Yukon. This information is necessarily in part of an approximate character; but most of it, it is thought, will prove a useful addition to our knowledge. Dr. Everette will now devote himself to the preparation of a work on the Yukon district of Alaska, for which his notes, charts, and collections will afford abundant material.

— The steamer *St. Paul* announces the wreck, July 31, of the bark *Montana*, in the Nushagak River, Bristol Bay; but it is believed no lives were lost. It is feared that by this disaster the Moravian mission to Nushagak may have lost part of its supplies or outfit.

— Lieut. Purcell of Stoney's expedition to the Kowak, or Kuak, river of the Kotzebue Sound region, has returned, disabled by illness. He gives the following notes on the progress of the expedition. The passage from Unalashka was extremely slow, owing to light winds; but *St. Michaels* was safely reached, and three natives and nineteen dogs were obtained, and the party proceeded to *St. Lawrence Bay*, where skin-clothing was purchased for winter use. The steam-launch *Viking* will be used in exploring the Kowak River, and birch canoes used when the limit of launch-navigation is reached. The expedition hoped to explore some two hundred and fifty miles of the river before going into winter quarters; and the engine of the *Viking* is arranged so as to work a small saw-mill-attachment to cut boards for building the houses, etc. After October, exploration will be carried on by sledge-parties. In May, 1886, Lieut. Stoney proposes to return to Hotham Inlet and complete its exploration, and to ascend the Nunatak, or Noatak, river, which has hitherto only been examined to a distance of a few miles from its mouth. The members of the expedition were well and enthusiastic, and much may be expected from their researches. On the way up, *Bogosloff Island* and the *Grewingk volcano* were visited. There was less smoke than in 1884; and a small spit was making out to the northwestward of the island, but there were no other changes of importance.

— The September pilot-chart, issued by the Hydrographic office, contains several novelties appropriate to the season when tropical hurricanes come up to our coast from the West Indies. The weather-changes, indicative of an approaching cyclone, and the manoeuvres needed to avoid its centre, are printed

in the margin, with a storm-card for better illustration. In the latter, the winds are represented blowing in true circles, which certainly should be corrected. Besides the track of the cyclone which damaged Charleston on Aug. 24, 25, eleven others are added from former years, to give shipmasters an idea of the course followed by these storms in different parts of the ocean. The wrecked schooner, *Twenty-one Friends*, has been reported twelve times from April 14 to July 31, floating in the course of the North-Atlantic drift, from about latitude $40^{\circ} 20'$, longitude 55° , to latitude $50^{\circ} 20'$, longitude 27° ; thus averaging about thirteen miles a day to the east north-east. The successful use of oil in stormy weather is illustrated by a number of examples.

— Twenty-one pages of "Results of meteorological observations made at the U. S. naval observatory during the year 1881" are recently published. The reductions were made under the direction of Lieut. Wilson, Professor Eastman having been relieved from charge of the department at his own request. The tri-hourly observations, even of the clouds, were made by the observatory watchmen, "who have acquired such a degree of skill as insures a reasonable accuracy in their work." The same can hardly be said of workers on the reductions, as mistakes are very numerous. The first half of table II. occupying half a page, when corrected of its more visible errors, looks like a severely treated proof-sheet. The form of publication is also peculiar: under temperature of the air, the columns marked 'highest' and 'lowest' do not contain the records of the maximum and minimum thermometers, but show only the highest and lowest of the tri-hourly readings; the true maxima and minima being inconveniently set apart in a special table. In table V. we find the lowest temperature for August by minimum thermometer to be 60° on the 14th, while the tri-hourly observations give it as 57.5° on the 19th. Looking back to the daily records, we see that the minimum for the 14th is not 60° but 66° , while the real minimum for the month is 56.5° on the 19th. Again: the mean minimum for May is given as 56.9° ; the mean of the lowest tri-hourlies is 53.6° . The tri-hourly readings are called 'hourly' in all the tables, and the monthly range of daily means is called 'extreme mean range for the month.' No reduction except averaging is made for the wet-bulb thermometer, so that all humidity factors have yet to be worked up. For January and December, the relative humidity from the wet and dry bulb monthly means comes out ninety-five and ninety-one per cent, which seems hardly possible even in the naval observatory. In future numbers of the Results, a considerable increase in accuracy over this one might reasonably be looked for; and much convenience would be gained by the use of bold-face and hair-spaced type to indicate the highest and lowest values in every column of records. The care given to the thousandths of an inch in the rainfall-data might be transferred to a determination of the accuracy of the four-inch rain-gauge.

— Mr. W. F. Denning of Bristol, Eng., directs at-

tention to the fact, that, toward the end of November next, the circumstances appear to be extremely favorable for a recurrence of the slow meteors from Andromeda, which formed such a fine display in 1872, and which had been previously observed by Heis in 1847, by Flaugergues in 1838, and by Brandes in 1798; for if Biela's duple comet still exists in any considerable degree of condensation, and if it has preserved nearly the same orbit as during the years 1826-52, it will be in perihelion only a short time before the earth crosses the node; and the conditions for a meteoric display will be even more auspicious than in 1872. The question, however, arises, whether planetary perturbations may not have so disturbed the orbit as to have materially altered the periodic time, and to have otherwise so modified the elements as to render the meteor-shower no longer visible, at least in its best aspect, from the earth. There was no sign of the shower in 1879, and the ensuing November ought to settle the question of its continued existence. If there is no shower of these meteors in November, their absence may be interpreted as strong evidence that the meteor-system was so disturbed by contact with the earth in 1872, as to have suffered considerable modification of its orbit.

— After most careful researches, extending over many years, Professor Karl Pettersen has arrived at some very important conclusions concerning the formation of the fiords in northern Norway, which are published in *Nature*. From his studies at Balsfjord, he has concluded that the granite blocks upon the surface were carried along the level of the sea on drift-ice when the sea was about one hundred and twenty feet higher than at present. A sharply defined line at this height, above which no blocks are found, seems sufficient to prove this; for, if the drift was deposited by a slowly gliding inland glacier, there would be no such line. He has therefore come to the conclusion, "that the Balsfjord is not of glacial origin, but formed an incision or depression in the mountain of older origin than the glacial age." He further believes that this conclusion may, in the main, apply to the question of the formation of all fiords in the north of Norway.

— The island of Formosa, which the French have just conquered, is very rich in vegetable and animal life, on account of its excellent climate. This island, though but three hundred and sixty kilometres long, and one hundred and forty wide from east to west, is the chief source of our camphor supply. The tree (*Laurus camphora*) grows in Japan, China, Sumatra, and Borneo also, but nowhere in such abundance as on Formosa. In order to obtain the camphor, the wood is cut into small bits, and placed in a crucible, from which the vapors are distilled over by a gentle heat, and collected upon a network of rice-stems.

— Since 1813 pieces of native iron have been brought from Greenland by many explorers, and have, in nearly every case, been ascribed to meteoric origin. Steenstrup, in his third voyage to Greenland (1876-1880), however, found the iron native in a basaltic rock at Asuk, in grains varying from a fraction of

a millimetre to eighteen millimetres. It is also found on the western and northern sides of Disko Island, and in other places. This settles beyond a doubt the question of the origin of the Greenland native iron, and the ore may be of great commercial importance in the future.

— The signification of the names of some Indian mountains of great height may be of interest. Kinchinjunga is the highest of a group of eight principal peaks, which rise from a mountain mass of which the view from Darjiling occupies about sixty degrees of the horizon. Its name is a compound and corruption of four Tibetan words, — *kung*, 'snow,' *chen*, 'great,' *jo*, 'masses,' *nga*, 'five' or 'several;' in short, the five great snowy peaks. The word 'five' is in this case not explicit, but indicates, so to speak, a handful. Mount Everest of British maps, supposed by many geographers to be the highest mountain on the globe, has a more attractive proper name in Tibetan, — Deva-danga, 'God's home,' usually shortened in conversation into Deodanga. Dobola-giri, the 'white mountain,' rises about one hundred miles westward from Deva-danga, though the name has been mistakenly applied to one of the high summits of the Kinchinjunga uplift. It is found on most maps as Dhaolagiri or Dwahlagiri; while Deva-danga is often found inscribed with its Indian appellation, Gaurisanka.

— Hans Kaan's study of hypnotism (*Ueber beziehungen zwischen hypnotismus und cerebraler blutfüllung*. Wiesbaden, *Bergmann*, 1855. 35 p. 8°) is a maiden effort, in which, *more germanico*, a single mustard-seed of observed fact is wrung and pressed, and considered in so many lights that a bay-tree of letter-press grows out of it. The fact in this instance is, that a certain very perfect hypnotic subject was awakened immediately out of her 'lethargic' state by the application of a hot compress to the head, whilst a cold compress had rather the opposite effect. In her 'cataleptic' state, on the contrary, a hot compress was without influence; while a cold compress soon brought her into the less 'deep' lethargic state, whence by the hot compress, she could be brought immediately into the normal condition. The experiment was repeated, with identical results, more than sixty times. The considerations spun upon it are relative to the condition of the blood-distribution in the brain during the two states. Experiments with the plethysmograph were instituted; and after a discussion, to which the author's lack of literary ability gives an almost impenetrable obscurity, the following conclusions are advanced as probable: that the hypnotizing passes, etc., produce a reflex anaemia of the cerebral cortex, out of which a hyperaemia gradually develops; and that these states successively pass downwards to the basal ganglia. In the main, Dr. Kaan's observations agree with those of Tamburini, and with the effects of amyl-nitrite, which seems to deepen, rather than relieve, the hypnotic state. It is made more and more probable that the cataleptic state, which is rare, and may be considered a sort of climax of trance, is accompanied by an intense hyperaemia of the brain.